BARRIERS TO CONTRACTING IN VILLAGE ECONOMIES: A TEST FOR ENFORCEMENT CONSTRAINTS*

RYAN BUBB†, SUPREET KAUR‡, AND SENDHIL MULLAINATHAN∓

DRAFT: PRELIMINARY AND INCOMPLETE

July 20, 2016

Abstract. In developing countries, informal contracting between individuals underlies activity in many markets, such as credit, savings, insurance, land, labor, and irrigation. This paper tests for a potential barrier to such contracting: enforcement problems. We offer to subsidize the cost of irrigation between potential water buyer and seller pairs, with the subsidy payment to be delivered three months in the future. We vary the seller's ability to enforce collection of payment by randomizing whether the subsidy payment would be delivered into the hands of the water buyer, or directly into the hands of the water seller. Under the Coasian benchmark, the amount of trade should not be affected by which party will receive the subsidy. However, consistent with enforcement constraints, the amount of irrigation is 58% higher under the Seller-subsidy relative to the Buyer-subsidy. Sellers use ex ante transfers—price discounts and trade credit—to induce trade under the Seller-subsidy, but not under the Buyer-subsidy. In contrast, there is little ex post sharing of the subsidy. The magnitude of the enforcement failure is economically meaningful: buyers experience a 0.274 standard deviation increase in crop yields when the subsidy is delivered directly to the seller rather than to themselves. There is little evidence that potential correlates of relational contracting—such as previous trading history or being in the same caste—equalize trade under the two treatments. The findings suggest that within the context of our experiment, contract enforceability is a first-order impediment to realizing the gains from trade.

---

* We thank Emily Breza, Rocco Machiavello, Bentley MacLeod, Jeremey Magruder, Ameet Morjaria, and Jann Spiess for their helpful comments. We are grateful to the Institute for Financial Management and Research (IFMR) and ICICI Bank for assistance with field research. Dominik Bulla, Nicole Brunda, Sanchit Kumar, and Evan Plous provided excellent research assistance. This project received financial support from Harvard University, the IFC, and IFMR. Any errors are our own.

†School of Law, New York University. Email: ryan.bubb@nyu.edu.

‡Department of Economics, UC Berkeley and NBER. Email: supreet@berkeley.edu. (Corresponding author).

∓Department of Economics, Harvard University and NBER. Email: mullain@fas.harvard.edu.

1. Introduction

The economic lives of the poor are characterized by informal contracting among individuals. In village economies, this is true of the markets for credit, savings, insurance, land, labor, and capital inputs such as irrigation. For example, to the extent that the poor access credit or insurance, it is almost exclusively through neighbors, relatives, and local moneylenders (e.g. Banerjee and Duflo 2007, Collins et al. 2010). Similarly, almost all agricultural land and labor transactions are informal bilateral contracts between co-villagers (e.g. Shaban 1984, Dreze and Mukherjee 1989). Barriers to interpersonal contracting therefore have the potential to substantively affect output and welfare in poor countries.

This paper examines one such barrier: enforcement constraints. When contract enforcement is imperfect, concerns about ex-post reneging can prevent two parties from engaging in mutually beneficial trade. This problem has potential relevance for any transaction involving temporal distance—where one individual delivers a good, service, or payment before the other must honor her part of the exchange.

While formal mechanisms may be limited in poor countries, a large theoretical literature emphasizes that the enforcement problem can be solved through informal mechanisms such as relational contracting. Rural villages—with their strong social linkages, low mobility, and repeat interactions across time and multiple markets—exemplify conditions where relational contracts have the potential to flourish. This presumption underlies a large body of work in development—for example, as a prerequisite for whether communal risk sharing could serve as a substitute for formal insurance (e.g. Townsend 1994). Consequently, to the extent that enforcement problems persist in this setting, this has potential implications for a wide array of economic activity, including in each of the markets discussed above.

In this paper, we design a simple test for enforcement constraints and examine whether their magnitude is economically meaningful. The backdrop for our test is village irrigation markets in India, in which smallholder farmers purchase water from a neighboring well owner each year.[1] To enable a test for enforcement, we introduce a cash subsidy for buyer-seller pairs. Specifically, if the seller delivers irrigation to the buyer during the hot season, then the pair receives the subsidy.[2] However, this subsidy is paid out well after the irrigation takes place—a month after the hot season ends.

This temporal lag in subsidy delivery gives us a lever with which to manipulate enforcement: we randomize in whose hands the future subsidy payment will be delivered. Specifically, in the Buyer-subsidy treatment, we tell the buyer-seller pair that the money will be delivered into the hands of the water buyer. In the Seller-subsidy treatment, the

[1]Because it is costly to transport water over long distances, water buyers can effectively only purchase water from a neighboring farmer whose land (and therefore well) is in close physcial proximity to their own. A given water buyer typically has 1-5 potential sellers from whom he could purchase water. Each buyer purchases water from his neighbors multiple times every year.

[2]The subsidy amounts to 50% of the market price of a typical irrigation.

pair is told the money will be delivered directly to the water seller. Note that the timing of events, information available to the parties, the amount of liquidity in the relationship, and the total surplus from trade are all exactly the same across both treatment conditions. The only difference is into whose hands the subsidy money will arrive. In the first treatment condition, the seller must trust the buyer to transfer funds to him, whereas the second condition ensures the money arrives directly to the seller.

Under the Coasian benchmark of perfect enforcement, the buyer and seller will ex ante agree on how to split the subsidy payment when it arrives, and there should be no difference in the level of trade across the two subsidy treatment conditions. In contrast, if the seller cannot trust the buyer to transfer (some subset of the) future subsidy payment to him, then he may be unwilling to bear the cost of providing irrigation up front. Thus, in the presence of enforcement constraints, the amount of irrigation will be higher under the Seller-subsidy relative to the Buyer-subsidy.[3] In addition, by examining the resultant impact on output (i.e. yields), we can assess whether the magnitude of enforcement failures is economically meaningful.

While this design enables us to construct a clean test for enforcement, its interpretation requires an important caveat. Because the subsidy increases the gains from trade, it induces irrigations that may not be efficient in its absence. We therefore cannot presume that trades that occur under the Seller-subsidy should occur in equilibrium. However, our goal is not to assess the underlying efficiency of the irrigation market. Rather, we use this setting—which involves trade among neighbors who buy and sell irrigation multiple times every year—as a convenient backdrop for our test.

To implement the experiment, we identify a random set of 431 water buyer-seller pairs across 21 villages in central Uttar Pradesh, India.[4] 94% of buyers purchased irrigation from a neighbor in the previous year, and 63% of pair members have traded with each other in the

[3]Could a mechanism other than enforcement constraints (or contracting failures more generally) generate a difference in trade if the subsidy is delivered to the seller instead of the buyer? For example, what if sellers are more likely to trust that we will deliver the subsidy payment than buyers? In this case, the sellers in both treatment groups would be more likely to believe that we will deliver the money as promised; if there are no enforcement constraints, then, based on their respective beliefs, buyers and sellers will agree to an ex post division of the subsidy, and outcomes should look no different in the two groups. Similarly, if the buyer has a higher alternate use of funds (e.g. due to a negative shock), this will be the case in both treatment groups; as long as the seller trusts that the ex post division will be as promised, in whose hands the money arrives will not matter. In addition, note that a failure in barganing (e.g. Meyerson Satterthwaite 1983) also caanot generate our predicted pattern in the absence of enforcement problems. For example, suppose the parties have private valuations to buying/selling irrigation; whatever division of surplus enables trade in the Seller subsidy group should be replicable under the Buyer subsidy group as long as the seller can contract with the buyer to transfer the subsidy ex post. In short, if there are gains from trade in the case of the Seller-subsidy treatment, then these gains exist in the Buyer-subsidy treatment; outcomes on the amount of trade should look the same as long as the seller can trust the buyer will split the subsidy as promised.

[4]The sample was constructed as follows. We identified 431 farmers who own a plot of land without a well on it (water buyers). For each of these farmers, we identified all the well owners around their plot of land who were physically close enough to sell them water; from this group, we randomly chose one potential water seller. 63% of the buyers in our sample had purchased water from their paired seller before our intervention.

past. These pairs are randomized into one of three treatment groups: the Buyer-subsidy, Seller-subsidy, and a pure Control group where no subsidy is offered. The subsidy offer applies to irrigations during the hot season (April-June), and subsidy payments delivered in July.

The effects on the level of ex ante trade point to the presence of enforcement constraints. The probability that a buyer-seller pair trades in a given week is 3.5 percentage points (30%) higher under the Seller-subsidy relative to the Buyer-subsidy.[5] Overall, Seller-subsidy pairs engage in 58% more hours of irrigation relative to those in the Buyer-subsidy.

This difference in irrigation has a substantial impact on output. On average, water buyers have crop yields that are 0.274 standard deviations higher when the subsidy is delivered to the seller rather than to themselves. This magnitude corresponds to an estimated 6% increase in crop profits. These findings indicate that within the specific context of our experiment, the enforcement failure among the Buyer-subsidy group (relative to the benchmark of the Suyer-subsidy) is economically meaningful.

We next turn to examine the details of contract terms to obtain supporting evidence for how enforcement constraints affect the structure of trade. We find that pairs seldom enter into contracts involving an ex post transfer of the subsidy. Specifically, among those pairs that receive a positive subsidy payment, in 87% of cases, subsidy recipients and their trading partners agree ex ante that the subsidy recipient will not transfer funds to the opposite party after the subsidy is delivered. This is consistent with anticipated costs to enforcing ex post contracts, leading people to not enter into such contracts in the first place. While only suggestive evidence, this is is in line with our hypothesis that the Buyer subsidy will be less successful at inducing ex ante trade because sellers do not trust buyers to transfer funds to them once the subsidy is delivered.

If ex post transfers are costly to enforce, then parties can use up front payments at the time of irrigation to enable trade to occur. Consistent with this, in the Seller-subsidy, sellers offer a price discount to encourage trade—in anticipation of the fact that they will receive a subsidy payment from us in the future. Specifically, in the Seller subsidy group, sellers are 8 percentage points (97%) more likely to give their paired buyer a price discount than those in the Buyer subsidy group. They are also more likely to offer trade short term credit—allowing buyers to repay after the irrigation date.

This highlights an important role for liquidity. In the presence of enforcement constraints, whichever party will receive the subsidy will be motivated to make an ex ante transfer (in order to satisfy his partner's participation constraint). The ability to make such a transfer up front, however, will depend on agents' wealth at the time of irrigation. Consistent with this, we find that the Seller subsidy is especially likely to increase trade (relative to the

[5]We measure the amount of irrigation via weekly surveys. Our field staff visited each buyer and seller every week over the course of the 3 month irrigation period (hot season), and verified reports of irrigation by checking soil moisture on the buyer's plot. We also conducted an endline survey to obtain information on yields and other endline outcomes.

Control group of no subsidy) when the seller is wealthier. Similarly, the Buyer subsidy is especially likely to increase trade (relative to the Control group) when the buyer is wealthier.

Finally, we examine potential correlates of informal enforcement mechanisms—including previous trading relationships between the buyer and seller, market power, or being within the same caste network. Overall, we find little evidence that correlates of relational contracting enable pairs to fully overcome the enforcement problem. Even in cases where social linkages are high, the Buyer subsidy group generally trades less than the Seller subsidy group. Overall, these results suggest that the participants in our study have limited ability to overcome the enforcement problem, at least within the context of our study.

While these correlates do not seem to enable pairs to overcome the enforcement problem, we do see evidence that they matter for contracting more generally. In the subsidy groups overall, 62% of pairs never engage in any trade. This suggests that for these pairs, a 50% subsidy was not sufficient to make trade worthwhile. Correlates of social and market linkages—e.g. whether the buyer and seller are the same religion or if they have traded in the past—have strong predictive power for which pairs end up taking advantage of the subsidy offer (versus having the same level of trade as the control group). These heterogeneous effects could just reflect efficient contracting. Alternately, they could be indicative of a more general fundamental contracting failure—with transactions costs increasing so much with social distance that even a 50% subsidy is not enough to induce trade on the margin.[6] While our experiment is not well suited to examining the underlying cause for this pattern of results, it constitutes an interesting direction for additional research.

This paper contributes to the empirical literature on contracting failures in developing countries. A growing number of studies points to the relevance of enforcement issues (McMillan and Woodruff 1999; Machiavello and Morjaria 2014, 2016; Machiavello and Miquel-Florense 2015). These papers apply clever identification strategies to contract data to examine the role of relational contracting among formal firms. They find support for the idea that repeat relationships play an important, though imperfect, role in enabling transactions among firms. [7]

Our study design complements these analyses. Using the Seller-subsidy case as a benchmark for how much irrigation could occur under the subsidy, we can quantify the extent to which the Buyer-subsidy group falls short. The effects on irrigation and yields indicate that the enforcement failure is large in magnitude. Of course, this magnitude applies specifically to the context of the trading opportunity we create through the subsidy offer. However,

[6]This is consistent with Anderson (2011), who presents evidence that caste differences are correlated with lower levels of irrigation transactions in Indian villages—with substantial implications for the yields of low caste farmers.

[7]In addition, a small set of studies explores other, but related, contracting issues. Banerjee and Duflo (2000) present evidence that reputation is used to solve incomplete contracting problems in the Indian software industry. Iyer and Schoar (2008) use a field experiment to examine how concerns about hold-up affect the timing of payments and ex-post barganing.

the fact that such large failures arise in our population—neighboring farmers who routinely buy and sell irrigation, and who have traded with each other in the past—suggests that enforcement problems may not be limited to just the narrow case of our subsidy offer. For example, these findings may be relevant in understanding why we rarely observe long-term trade credit in irrigation markets in equilibrium.[8] More generally, these findings suggest that enforcement problems have the potential to hamper mutually beneficial trade in village economies, which are characterized by the prevalence of informal interpersonal exchange.

2. Model

2.1. **Set-up.** Assume an agent ("the buyer") has access to a production technology. In period 1, the agent can invest one unit of input, with a benefit b realized in period 2. There is a seller who can supply the input to the agent. Let c denote the cost of providing the input; this cost is borne by the seller upon delivery of the input (i.e. in period 1).

In addition, we introduce a subsidy payment, $s > 0$, that is delivered to one of the parties in period 2 *if* trade occurs in period 1. This subsidy increases the gains from trade. With the subsidy, the total surplus from trade is therefore $b + s - c$.[9] Trade is efficient if $b + s - c \geq 0$.[10]

Finally, let w denote the buyer's wealth at the beginning of period 1. This constitutes the level of cash on hand in period 1 that is available to the buyer for any up front payments to the seller.

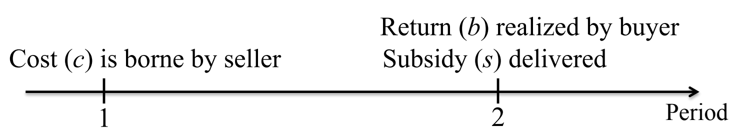

Figure 1. Model - Timing of Events

In what follows, we compare two cases. In the "Buyer Subsidy" case, the subsidy payment s is delivered into the hands of the *buyer* in period 2 if trade occurs in period 1. In the "Seller Subsidy" case, the subsidy payment s is delivered into the hands of the *seller* in period 2 if trade occurs in period 1. Our primary interest is in comparing how these cases impact whether trade occurs in period 1.

[8]For example, among the Control group (where no subsidy was offered), 97% of irrigation payments are due within one week of the irrigation date, and no sellers allow buyers to wait until harvest to repay.

[9]Without loss of generality, we ignore time discounting between periods for simplicity.

[10]In the experiment, the subsidy is imposed externally by us. From the perspective of the buyer and seller, it is part of the surplus they gain through trading. For the purposes of model, we therefore include s as part of the surplus that determines whether trade is efficient.

2.2. **Benchmark: Perfect Enforcement.** We first examine the benchmark case of perfect contracting. We assume that the buyer and seller can write a contract in period 1 over payments in period 2, and this contract is perfectly enforced in period 2.

2.2.1. ***Buyer Subsidy case.*** Let p_1^B and p_2^B denote the payments made by the buyer to the seller in periods 1 and 2, respectively, in the Buyer Subsidy case. Each of these must be weakly less than the buyer's cash on hand in each period

$$\begin{aligned} p_1^B &\leq w \\ p_2^B &\leq b+s+\left(w-p_1^B\right) \end{aligned}$$

The total payment that the seller received for the input is therefore $p^B \equiv p_1^B + p_2^B$. Throughout the model, we assume the seller recovers as much of the payment as possible at the time of sale in period 1, and recovers the rest in period 2.[11] The constraint on the maximum price that can be charged therefore collapses to:

$$p^B \leq w+b+s \tag{2.1}$$

The buyer will be willing to trade if: $b+s \geq p^B$. The seller will be willing to trade if: $p^B \geq c$. Thus, both parties' participation constraints can be satisfied iff:

$$b+s-c \geq 0 \tag{2.2}$$

If condition (2.2) holds, then any price in the range $p^B \in [c, b+s]$ gives both parties non-negative payoffs. In addition, note that any p^B in this range will also automatically satisfy condition (2.1). Thus, trade occurs if and only if condition (2.2) is satisfied.

2.2.2. ***Seller Subsidy case.*** Let p^S denote the price of the input in the Seller Subsidy case. The constraint on the maximum price that can be charged reduces to:

$$p^S \leq w+b \tag{2.3}$$

The difference with condition (2.1) arises from the fact that the subsidy will be paid to the seller in period 2.

The buyer will be willing to trade if: $b \geq p^S$. The seller will be willing to trade if: $p^S + s \geq c$. Thus both parties participation constraints will be satisfied iff:

$$b+s-c \geq 0 \tag{2.4}$$

The chosen price will be in the range: $p^S \in [c-s, b]$, giving both parties non-negative payoffs. As before, any p^S in this range will also automatically satisfy condition (2.3). Thus, trade occurs if and only if condition (2.4) is satisfied.

[11] Under perfect enforcement, the buyer and seller will only care about the total payment p^B. They will be indifferent about whether the funds are transferred in period 1 or period 2, since any amount that is decided in period 1 will be paid out with certainty. Thus, this assumption does not change the logic of the results, and allows us to collapse the two price constraints into one.

Note that the necessary and sufficient condition for trade to occur is *exactly* the same both the Buyer Subsidy and Seller Subsidy cases. Trade occurs as long as there is surplus that can be generated through trading. It is irrelevant who ultimately receives the subsidy—the agents can write an ex ante contract to split the gains from the subsidy to ensure both parties' participation constraints are satisfied. While the prices charged and division of surplus may be different in the two cases, this will not affect whether trade occurs. This is the essence of the Coase Theorem result in the absence of transactions costs.

In addition, note that the buyer's wealth or cash on hand is irrelevant—w does not enter in conditions (2.2) and (2.4). This is because, due to perfect enforcement, the seller can recover payment in period 2 from b as needed.

2.3. **Enforcement Constraints.** Now suppose a contract written in period 1 is not perfectly enforceable in period 2. As before, the buyer can make transfers to compensate the seller in period 1 (out of his personal wealth w) or in period 2. However, the seller can only recover funds in period 2 with some probability.

2.3.1. ***Buyer Subsidy case.*** As before, let p^B denote the total payment made by the buyer to the seller in the Buyer Subsidy case. From the perspective of the seller in period 1, the maximum that can be recovered from the buyer is now:

$$\begin{aligned} p_1^B &\leq w \\ E_1\left[p_2^B\right] &\leq \mu\left[b+\left(w-p_1^B\right)\right]+\lambda s \end{aligned}$$

where $\mu \leq 1, \lambda \leq 1$ denote the probabilities that the seller will be able to enforce payment of non-subsidy income and subsidy income, respectively, in period 2. We allow each of these probabilities to differ (though it is possible that $\mu = \lambda$). Recall our assumption above that the seller recovers as much of the payment as possible at the time of sale in period 1. Substituting for $p_1^B = w$ in the second inequality and adding the two conditions together, the constraint on the maximum expected payment that the seller can recover reduces to:

$$E_1\left[p^B\right] \leq w + \mu b + \lambda s \tag{2.5}$$

This condition is intuitive. The seller can recover up to w with certainty (since the buyer has this much cash on hand at the time of sale in period 1), and can enforce recovery in period 2 imperfectly.

The buyer's participation constraint is satisfied if: $b + s \geq E_1\left[p^B\right]$.[12] The seller's participation constraint is satisfied if: $E_1\left[p^B\right] \geq c$. In the Buyer Subsidy case, trade occurs if

[12]This assumes that the buyer also does not know with certainty whether the seller will be enable to enforce the contract in period 2. This may be possible if, for example, the parties would appeal to a remediation process with the village head to solve a dispute. An alternative approach would be to assume there is asymmetric information about period 2 enforcement: the buyer knows if he will renege or not, whereas the seller only knows the probability of reneging in the population. This approach would not change the substance of our predictions. As will become clear before, the binding constraint on enforcement comes from

and only if the following two conditions are met:

$$b + s - c \geq 0 \tag{2.6}$$

$$w + \mu b + \lambda s \geq c \tag{2.7}$$

Condition (2.6) is given by combining the buyer and seller's participation constraints. Condition (2.7) is given by combining the seller's participation constraint with the constraint on the maximum that can be recovered, given by (2.5).

Condition (2.7) is the key to understanding how imperfect enforcement in period 2 can prevent trade from occurring in period 1, even when there is positive surplus. If $\mu = \lambda = 1$ (this is equivalent to perfect enforcement), (2.7) is automatically satisfied whenever condition (2.6) is satisfied, and—as in the benchmark case—trade occurs whenever there is positive surplus. However, if μ and λ are sufficiently low, then the seller will not agree to trade in period 1 unless the buyer has enough cash on hand in period 1 to pay up front.

2.3.2. ***Seller Subsidy case.*** As before, let p^S denote the total payment in the Seller Subsidy case. From the perspective of the seller in period 1, the constraint on the maximum that can be recovered from the buyer is:

$$E_1\left[p^S\right] \leq w + \mu b \tag{2.8}$$

The buyer will be willing to trade if: $b \geq E_1\left[p^S\right]$. The seller will be willing to trade if: $E_1\left[p^S\right] + s \geq c$. In the Seller Subsidy case, trade occurs if and only if the following two conditions are met:

$$b + s - c \geq 0 \tag{2.9}$$

$$w + \mu b + s \geq c \tag{2.10}$$

These conditions are the same as those in the Buyer Subsidy case, *except* for an important difference between the enforcement constraint conditions ((2.7) vs. (2.10)). In the Seller Subsidy case, the seller is sure of receiving s in period 2. In contrast, in the Buyer Subsidy case, the buyer will receive s if trade occurs, but the seller's ability to recover it is only $\lambda s < s$.

If $\lambda = 1$ (no enforcement constraints), then the level of trade the Buyer and Seller subsidy cases will be exactly the same. However, if $\lambda < 1$, condition (2.10) will be marginally more likely to bind than condition (2.7). Consequently, under enforcement constraints, the level of trade in period 1 will be lower in the Buyer subsidy case than the Seller subsidy case. This is the key prediction of our model.

---

the seller's participation constraint and beliefs. The model's predictions would therefore essentially remain the same.

Conceptually, the introduction of the subsidy gives us a lever to manipulate ex post enforcement levels—through our ability to vary which party receives the subsidy. In the experiment, we randomize the recipient of the ex post subsidy and test whether this affects the probability of ex ante trade.

Note that our test only has power to detect enforcement constraints if w and μ are sufficiently small. For example, if the buyer has enough cash on hand to pay up front in period 1 then the value of λ is irrelevant because the enforcement constraints will never bind. In this case, trade would be the same in both cases, even though there is an underlying enforcement problem. In addition, the model implicitly assumes that the seller has the liquidity to cover the cost of c up front; if the buyer and seller cannot cover the costs of irrigation between them, then then trade may not occur in either the Seller or Buyer subsidy cases, and we may not be able to detect enforcement failures even if they exist.

2.4. **Discussion - Alternate Explanations.** Could a mechanism other than enforcement constraints generate a difference in trade if the subsidy is delivered to the seller instead of the buyer? In the experiment, one potential consideration is that sellers and buyers have different beliefs about whether we will return to deliver the subsidy payment. Suppose sellers are more likely to believe we will return than buyers on average. In this case, the sellers in *both* treatment groups would be more likely to believe that we will deliver the money as promised; if there are no enforcement constraints, then, based on their respective beliefs, buyers and sellers will agree to an ex post division of the subsidy, and outcomes should look no different in the two groups.

Similarly, if the buyer has a higher alternate use of funds (e.g. due to a negative shock), this will be the case in both treatment groups; as long as the seller trusts that the ex post division will be as promised, in whose hands the money arrives should not matter. Alternately, if bargaining power is affected by who receives the funds, this may affect the ex post division of surplus but should not affect whether trade occurs. In short, if there are gains from trade in the case of the Seller-subsidy treatment, then these gains exist in the Buyer-subsidy treatment; outcomes on the amount of trade should look the same as long as the seller trusts the buyer will split the subsidy as promised.

Finally, the model above assumes perfect and symmetric information among the buyer and seller—specfically, that b is known with certainty. However, more general forms of incomplete contracting (aside from enforcement constraints) could prevent trade from happening even when $b > c$. In the experiment, such problems will be common to the Buyer and Seller subsidy groups. By introducing an external subsidy amount s and explicitly informing both parties about s, we are able to ensure that information about s is symmetric. In general, one could write a more complicated incomplete contracting model where altering who receives the subsidy leads to differential trade. Consequently, we recognize that the most defensible interpretation of our design is a test for contracting failures or

transaction costs. We view our intervention as being most consistent inducing a change in enforcement, and in the exposition, we will use the language of enforcement in what follows for concreteness.

3. Experiment Design

3.1. **Context: Groundwater Markets.** We test for enforcement constraints in the context of spot markets for groundwater in the central/eastern region of the state of Uttar Pradesh, India. In this area, groundwater is the predominant source of irrigation water for agriculture. The fixed cost of sinking a borewell and purchasing an engine to pump the water out of the ground is fairly large. Borewells are therefore typically owned by wealthier farmers in a village.

Farmers who do not own their own well can purchase irrigation from a well-owner on a neighboring plot of land. There are extremely active spot markets for groundwater in the region. Buyers typically rent another farmer's borewell and engine at an hourly rate. This rate includes the variable cost of diesel, which must be used for the engine to pump water. The diesel cost accounts for about 50% of the typical cost of pumping water. 99% of the water transactions in our baseline survey sample were these hourly spot contracts (in contrast with season-long irrigation contracts which are prevalent in other parts of India).

While irrigation purchases can happen all year-round, the peak season for groundwater sales is from May-June—the hottest months of the year. Farmers who choose to grow crops during this time of year—particularly sugarcane—must irrigate their crops to prevent them from drying out. Smallholder farmers claim that they have trouble irrigating as much as they'd like during this period due to liquidity constraints.

Water is transported from the well via cheap plastic hoses that can be attached to the well and run to the desired plot of land. Because there is loss in water from transporting over long distances, farmers typically only purchase water from someone on a nearby plot of land. Water buyers typically purchase water from a neighboring farmer multiple times each year. This is therefore a setting with a high degree of repeated interactions: buyers and sellers are neighbors and will be for their entire lives (given limited mobility and extremely low levels of land sales). Most farmers have access to 1-5 potential sellers.

Figure **??** suggests that 99% of water buyers perceive the net returns to an additional irrigation on agricultural profits to be positive. Most perceive the magnitude of the returns to be fairly high.

3.2. **Sample Construction.** We identified potential water buyer-seller pairs in 21 villages. In each village, we constructed a census of cultivators. We identified potential "water buyers" as farmers who cultivated a plot of land without a well on it, and randomly picked a subset of these in each village. For each of these chosen water buyers, we identified all neighbors with a borewell and pump engine who were close enough to potentially sell water

to that buyer; we randomly picked one of these potential sellers. Any given household could only be a part of one pair. Through this process, we created 431 unique water buyer-seller pairs in every village.[13]

3.3. **Treatments and Randomization.** We use a simple intervention to test for enforcement constraints. We encouraged buyer-seller pairs to trade by offering to pay them a subsidy each time the buyer purchased irrigation from the seller over a 3 month period that encompasses the main irrigation season. The participants were told that the subsidy payment would be delivered *after* the end of the irrigation season, in July (this is when the monsoon arrives, and irrigation purchases generally become unnecessary). The subsidy was substantial in size—constituting about 50% of the cost of a typical irrigation.

Each pair was randomized into one of the following treatment groups:

(1) Seller Subsidy: Subsidy payment delivered into the hands of the water seller.
(2) Buyer Subsidy: Subsidy payment delivered into the hands of the water buyer.
(3) Control: No subsidy offered.

Both members of the pair were informed together about the details of the subsidy offer—the amount, timing of payment delivery, and who it would be delivered to—in March, before the start of the irrigation season. We stratified randomization by village, with 40% of pairs within a village assigned to each of the two subsidy conditions and 20% of pairs assigned to the Control group.

Given the magnitude of the subsidy, the gains from trade are substantially higher in Groups 1 and 2 than in Group 3. We therefore would expect that the subsidy groups (Groups 1 and 2) would irrigate more than Group 3.

Our primary interest is in comparing the level of trade between the two subsidy groups. Treatments 1 and 2 mirror the two cases in the model. The seller bears the cost of irrigating (in terms of diesel, his time, and possible depreciation of the engine) at the time of irrigation. The buyer can compensate the seller for these costs at the time of trade, or potentially defer some part of the payment until a later date (delivery of our subsidy 3 months later, or after harvest). Note that the total surplus from trade, timing of events, information available to each party, and liquidity available at the time of trade is exactly the same in both Groups 1 and 2. The only difference is whether the seller is assured of receiving the subsidy payment directly, or whether it goes to the buyer—creating a potential recovery issue. If the buyer and seller can agree at the time of trade on how to divide the subsidy, and expect that both parties will follow through on this without reneging when the subsidy arrives, then there

[13] We initially created 449 pairs. However, 18 pairs are excluded due to operational oversights. In 16 cases, a water seller household in one pair was assigned as a water buyer household in another pair. This occurred because landholdings are fragmented, with most households owning multiple plots of land—with borewells on only a subset of them given their prohibitive cost. In addition, in one pair, a household that was chosen as a water seller owned a well, but did not own an engine, and so could not actually sell water. Finally, in one case, the water seller migrated out of the village and did not participate in the experiment.

should be no difference in the amount of trade during the irrigation season between Groups 1 and 2. However, if there is a chance that the buyer will renege, then the level of ex ante trade during the irrigation season will be higher under the Seller Subsidy than under the Buyer Subsidy.

3.4. **Timeline and Protocols.** Figure 2 summarizes the experiment timeline.

We approached buyer-seller pairs in early March. At this time, for each pair, we conducted a meeting that included the buyer, the seller, the elected village head (pradhan), and one of our field staff. For pairs in the subsidy groups, the field staff member explained the rules of the subsidy offer, as described above. For pairs in the control group, the field staff simply reiterated that the buyer and seller could potentially trade with each other during the upcoming irrigation season.

The purpose of having the village leader present at each sit-down was to build confidence that we would indeed return three months later with the subsidy payment as promised. In addition, as discussed below, we built trust with participants in two additional ways. First, we had conducted baseline surveys several months earlier in the villages where the experiment was conducted, and households were paid for their participation. Many participants were therefore familiar with us and had received money from us in the past. Second, our staff visited the buyer and seller every week during the irrigation season, making them a regular and familiar presence in the village while the experiment was being conducted.

Any irrigations conducted between April and June were eligible to count for the subsidy payments. While the peak irrigation season is in May-June (the hottest months of the year), we included April in the subsidy window, since this is when irrigations could potentially begin. Buyers and sellers could irrigate as many times as they wanted during this period, with a lump sum subsidy amount s earned for each irrigation instance. The participants were told that the total earned subsidy payments would be delivered in cash in the beginning of July (to the buyer or seller, based on the pair's treatment assignment). Harvest for sugarcane—the predominant cash crop in the area, and the crop for which irrigation is most frequently purchased during this time—occurs starting in October and continues until the following January. The payoff to irrigation, in terms of crop revenue, would therefore be realized 3-6 months after the end of the irrigation season.

3.5. **Data.** To accurately measure irrigation levels, we surveyed each pair weekly during the irrigation season. Every week, our surveyors visited each buyer and seller separately to ask them if they irrigated; if they both reported they had, the staff walked to the buyer's plot to verify irrigation by checking the soil moisture. They also collected information on the number of hours of irrigation purchased, the price charged, and the date at which payment was made or was expected to be made.

A year after the intervention, we returned to perform an endline survey. In this survey, we collected additional outcome variables, including crop yields and the amount of irrigation

that had been purchased from other potential sellers (aside from the paired seller). We managed to locate and survey all except 1% (12) of the 862 water buyers and sellers in our sample. This gives us complete endline data for both the buyer and seller for 419 pairs, or 97% of our sample. For consistency in analysis, we limit the sample in the main tables of the paper to these 419 pairs.

Finally, we have two sets of basic demographic and baseline measures. The first set was collected from the full experiment sample. In addition, we conducted a fuller, more detailed baseline survey for a subset of the households in the experiment. This data is from an extensive survey that was conducted in many villages in the area a year before the intervention. This survey was part of a broader project. 61% of the buyers and sellers in the experiment were part of this full baseline survey sample.

Table 1 provides summary statistics and balance checks on baseline covariates in the sample.

3.6. **Estimation.** To test for enforcement constraints, we estimate:

$$y_{ij,t} = \beta_0 + \beta_1 SellerSubsidy_{ij} + \beta_2 BuyerSubsidy_{ij} + \delta_v + \mathbf{X_i'}\theta_\mathbf{i} + \mathbf{X_j'}\theta_\mathbf{j} + \varepsilon_{ij,t}, \tag{3.1}$$

where $y_{ij,t}$ is the amount of irrigation between buyer i and seller j in week t of the experiment period (i.e. the irrigation season). $SellerSubsidy_{ij}$ and $BuyerSubsidy_{ij}$ are dummies for whether buyer-seller pair ij was assigned to the Seller subsidy group or Buyer subsidy group, respectively. The omitted category in the regression is assignment to Control. The δ_v is a vector of village fixed effects (since treatment assignment was stratified by village), and $\mathbf{X_i'}\theta_\mathbf{i}$ and $\mathbf{X_j'}\theta_\mathbf{j}$ are vectors of baseline covariate controls for the buyer and seller, respectively.[14]

Under the null of perfect enforcement, we would expect $\beta_1 = \beta_2$. However, under imperfect enforcement, we would expect $\beta_1 > \beta_2$. In addition, since the subsidy increases the gains from trade relative to the Control, we expect $\beta_1 > 0$ and $\beta_2 > 0$ (regardless of whether there are enforcement constraints).

4. Results

4.1. **Take-up of the Subsidy.** Recall that the subsidy covers 50% of the market costs of the typical irrigation. Despite this incentive, 62% of buyer-seller pairs in the Subsidy groups never trade with each other (Figure 3). This suggests that the subsidy offer was not strong enough to enable trade in most pairs, despite baseline beliefs among water buyers that the returns to irrigation are high. This could be due to a variety of factors. First, this could reflect low actual returns to irrigation for buyers or capacity constraints among sellers. It could also reflect low perceived returns to our subsidy offer because people did not

[14]In the main specification, we control for each of the covariates in Table 1. For those controls where we do not have data for all participants (i.e. the 39% of participants who did not participate in the extensive baseline survey), we code those values as zeros and add dummies to indicate missing baseline data in the regressions. Below, we show robustness to various alternate control strategies.

believe we would deliver the money, despite our efforts to establish our credibility. Second, match-specificity in buyer-seller pairs—due to the irrigation technology or due to barriers to contracting between certain individuals—could prevent trade for some of our randomly matched pairs. Third, even if a pair wanted to trade, liquidity constraints could prevent trade from happening—since either the buyer or seller need to bear the up front cost at the time irrigation occurs. Below, we offer suggestive evidence that the second and third considerations may play a role in preventing take-up of the subsidy by some pairs.

4.2. **Ex-ante Trade: Effects on Irrigation.** We begin by plotting the simple average of hours of irrigation purchased in each week of the experiment, separately for each treatment group (Figure 4). The amount of irrigation picks up for all groups after week 5—denoting the start of the main irrigation season in May, when extreme heat begins. Irrigation purchases taper off by week 14, as the monsoon onset begins in early July. The figure shows that on average, both Subsidy groups irrigate more than the Control group. In addition, as predicted under enforcement constraints, the Seller Subsidy group irrigates more than the Buyer subsidy group on average.

Table 2 shows estimates of specification (3.1). Panel A examines trade during the irrigation season. Col. (1) provides OLS estimates.[15] Because of the large percentage of zero values in the hours of irrigation, we estimate a tobit model in Col. (2) of Table 2 and report marginal effects. Consistent with the fact that the subsidy increases the returns to trade, the Subsidy group pairs trade substantially more than the Control group pairs. Relative to the Control, we estimate that the pairs in the Seller subsidy group irrigate 1.478 more hours each week and the Buyer subsidy group irrigate 0.872 more hours each week with each other during the main irrgation season (Col. 2).

In addition, the Seller subsidy group trades substantially more than the Buyer subsidy group: the difference in the hours of irrigation purchased is 0.632 hours per week in the main irrigation season (p-value of 0.02). The pattern of results in the full sample is similar: the Seller subsidy group irrigates 0.544 more hours per week on average. This magnitude corresponds to 58% of the Buyer subsidy group mean. Similarly, the Seller subsidy group pairs are more likely to irrigate at all in a given week than the Buyer subsidy group pairs (Col. 3). Finally, Col. (4) mixed evidence of extensive margin effects: the Seller subsidy group is more likely to trade at least once during the experiment than the Buyer subsidy group. In Panel B, we extend the sample to also include the month before the main irrigation season, with little qualitative change in the results.

4.3. **Ex-post Transfers: Sharing of Subsidy Payments.** Did subsidy pairs who traded share the ex post subsidy? When the subsidy payments were delivered, we asked each pair

[15] Appendix Table (A1) shows robustness to other control strategies. Note that in Col. (5), to determine which covariate controls to include in the model, we use the "post-double-selection" method recommended by Belloni, Chernozhukov, and Hansen (2012, 2014). The results are essentially the same as those in the main specification.

member separately how they intended to divide the subsidy payments. In all cases except one, the buyer and seller gave the same answer for whether the person receiving the subsidy would share any part of it with the opposite party. In addition, in instances where sharing was expected, both parties were also consistent in the amount they said would be shared with the exception of one case. This indicates that buyers and sellers had generally spoken in advance about how the subsidy would be split among them.

Among pairs that receive the subsidy, only 17% intend to share any of it with the opposite party (Figure 5). In the Seller subsidy group, in 75.4% of pairs, both parties said the seller would keep the entire subsidy. In the Buyer subsidy group, in 91.5% of pairs, both parties said the buyer would keep the entire subsidy.[16]

The limited amount of ex post sharing is consistent with barriers to payment recovery. In the presence of enforcement constraints, parties will ex ante not enter into contracts that involve ex post transfers. In the case of our subsidy offers, the overwhelming majority of those who traded expected the subsidy recipient to keep the payment in its entirety. This is consistent with higher ex ante trade in the Seller subsidy groups than the Buyer subsidy groups—since sellers did not expect that they would receive a transfer from the buyer when we delivered the payment.

4.4. **Ex-ante Transfers: Price Discounts and Credit.** If the subsidy recipient cannot credibly commit to transferring funds ex post, he could make an ex ante transfer at time of sale—via a change in up front price paid. Such transfers could be used to secure the other party's participation in trade. We examine features of contract terms to check for such transfers. Note that such analysis is necessarily suggestive: the model delivers clear predictions on the level of ex ante trade. However, the effects on contract terms reflects division of the surplus, for which clear predictions cannot be formed without taking a strong stance on the bargaining technology.

We begin with prices.[17] Table 3 examines the difference in prices charged for irrigation among the three treatment groups.[18] On average, buyers pay Rs. 27/hour less in the Seller subsidy group, relative to the Buyer subsidy group (Col. 1, p-value 0.026). Overall, they are 8 percentage points (97%) more likely to receive a discounted (Col 2, p-value 0.002).

[16]Unfortunately, we did not go back to verify if the subsidy was actually divided the way respondents said it would be. However, in cases where both parties said there would be no sharing, it is likely that no sharing occurred.

[17]We only observe prices charged in the 642 pair-week instances where trade actually occurred. If, on the margin, different types of pairs are selecting into trade in the two subsidy groups, then this will make it more difficult to interpret effects on prices. This caveat applies to any analysis of prices.

[18]There is usually a standard going hourly rental price for a borewell in each village. In our sample, this going price was either Rs. 70/hour or Rs. 60/hour. We use the modal hourly price in the Control group in each village to determine the going village rate. Very few observations deviate from this modal price. The market price for an irrigation is the hourly rate*number of hours irrigated. The dependent variable in Col. (1) is the Amount charged - Market price.

These findings are consistent with sellers lowering prices up front to induce the buyer to trade when they know they will be receiving the subsidy.

In addition, we also see evidence that in the Seller subsidy treatment, sellers are more willing to extend short term trade credit. Figure 6 shows the cummulative distributions of number of days before payment is due, conditional on an irrigation. The distribution for the Seller subsidy group is shifted to the right of the Buyer subsidy group. Note, however, the absolute duration of deferred payments is not large: all payments are due within 20 days of irrigation.[19] Table 4 is consistent with these findings, and indicates that sellers end up providing more trade credit to buyers in the Seller subsidy condition relative to both the Buyer subsidy condition and the Control group.

Finally, in Appendix Table A2, we look for one additional piece of evidence on costly action by sellers: crowd out of other potential buyers (i.e. buyers other than the one with which the seller was paired in the experiment). Panel A provides evidence that because they trade with their paired buyer, sellers in the Seller subsidy group are less likely to sell to other buyers in the village. ln contrast, there is no evidence of crowd out among sellers in either subsidy condition.

Together, the above findings suggest that sellers engage in a variety costly concessions or actions to take enable trade when they are assured the subsidy payment. The price reductions and trade credit may serve to increase the likelihood that the buyer's participation or liquidity constraints will not bind. While sellers appear willing to make such up front concessions under the Seller subsidy, we see little evidence they are willing to do so under the Buyer subsidy.

4.5. **Heterogeneity: Mediating Effects of Wealth.** The model predicts that the seller and buyer's wealth are potentially important for two reasons. First, someone must bear the cost of supplying irrigation at the time of sale (e.g., the diesel to run the engine). This is true regardless of whether there are enforcement costs. Second, in the presence of enforcement constraints, the ability to make ex ante transfers at the time of irrigation can enable the parties to take advantage of the subsidy offer. In this case, a wealth increase (i.e. increased cash on hand at the time of irrigation) will especially help the party that will receive the subsidy; this is because that individual is motivated to make ex ante transfers to enable trade.

Table 5 examines treatment effects based on baseline self-reported income. The cutoffs are based on the median income level of buyers in the sample. Col. (1) examines heterogeneous effects by the wealth of the seller. When sellers have are wealthier (income above median), there is some evidence that this increases the treatment effect of the Seller subsidy (Col. (1), significant at 10% level). Similarly, when buyers are wealthier, there is some evidence that this increases the treatment effect of the Buyer subsidy (Col. (2),

[19]These numbers show the payment due date on the date of the irrigation transaction. Unfortunately, we do not have data on whether each of these deferred payments was made on time.

significant at the 10% level). However, we cannot reject that the interaction terms on the Seller subsidy vs. Buyer subsidy treatments are different in this case. This pattern of effects provides some evidence that trade is boosted by wealth increases among the party who will receive the subsidy. This is consistent with the subsidy recipient being especially motivated to make ex ante transfers. For example, an increase in the seller's wealth may make it easier for him to cover the cost of the diesel or offer price discounts. Similarly, an increase in the buyer's wealth may make it more likely that the buyer can pay the full amount for the irrigation up front, making it unnecesary for the seller to provide credit. Such ex ante transfers are only possible when there is sufficient liquidity on hand at the time of trade—illustrating an important role for wealth in parties' ability to take advantage of the subsidy in the presence of enforcement constraints.

4.6. **Heterogeneity: Potential Correlates of Informal Enforcement Mechanisms.** A large theoretical literature in economics establishes ways in which relational contracting can enable agents to achieve the first best, despite lack of formal enforcement mechanisms. In Table 6, we examine potential correlates of informal enforcement mechanisms. We are interested in examining whether each correlate: i) plays a role in enabling pairs to take advantage of the subsidy offer in general, and ii) increase trade among Buyer subsidy pairs, reducing the difference between the Buyer and Seller subsidy. This latter set of effects would suggest that this correlate helps solve the enforcement problem. Overall, Table 6 provides limited support for the idea that the participants in our study had access to informal mechanisms that enable them to overcome enforcement constraints—at least within the context of our experiment (Panel B).

However, Panel A does provide some evidence that such correlates are meaningful for contracting more generally. Recall that only 38% of pairs in the subsidy groups engage in any trade with each other. The patterns in Panel A suggest that social distance—being the same religion or having a previous trading relationship—predicts which pairs manage to take advantage of the subsidy at all.

4.7. **Effects on Crop Yields.** Table 7 examines the reduced form effects of the subsidy treatments on crop yields. First, Col. (1) examines effects on total irrigation: the hours of irrigation purchased by water buyers from their paired seller or any other sellers. Relative to the Control group, the buyers in the Seller subsidy and Buyer subsidy groups increase total irrigation by 6.14 and 3.32 hours, respectively. These magnitudes are large, corresponding to 42% and 23% of the Buyer subsidy mean, but are not statistically distinguishable from each other (p-value of 0.117).

The remaining columns examine effects on crop yields. Crop yields is a composite index, standardized using the means and standard deviations of yields in the Control group. Relative to the Control group, buyers' crop yields in Seller subsidy increase by 0.392 standard deviations in the Seller subsidy condition (Col. 2, significant at 5%); the point estimate for

the Buyer subsidy condition is 0.118 but not significantly different from yields among the Control group. The buyers in the Seller subsidy group therefore experience an estimated 0.273 standard deviation increase in crop yields relative to those in the Buyer subsidy group (p-value 0.052). Col. (3) estimates effects off non-fallow plots, with qualitatively similar differences between the two subsidy groups.[20] To benchmark this magnitude, using baseline survey data on crop profits for the Control group, we estimate that a 0.3 standard deviation increase in the yields index corresponds to an approximately Rs. 1,336 or 6.4% increase in crop profits.

Since crop yields comprise the major source of annual income for the experiment participants, the magnitude of effects on yields is economically important. Buyers appear better off—at least by this measure—when the subsidy payment is delivered to the seller rather than to themselves. Within the setting of our experiment, the consequence of enforcement failures is economically meaningful.

5. Conclusion

We study enforcement constraints in a setting with a high level of repeated interactions: irrigation sales among Indian farmers with neighboring landholdings. Using a simple intervention, we subsidized the cost of irrigation between potential water buyer and seller pairs, with the subsidy payment to be delivered three months in the future. We randomized whether this payment would be delivered into the hands of the water buyer, or directly into the hands of the water seller. Consistent with enforcement constraints, when the parties know the money will be delivered directly to the water seller (rather than the water buyer), the amount of irrigation is 58% higher. Consistent with difficulty in recovering ex post funds, 83% of subsidy recipients do not share their subsidy payment with their paired buyer or seller. Rather, sellers use ex ante transfers (in the form of price discounts) to induce trade in the Seller subsidy treatment. The magnitude of effects on the amount of irrigation and yields suggest that within the context of our experiment, contract enforceability is a first-order impediment to realizing the gains from trade.

[20] For administrative reasons, plots were chosen by us in the previous year before planting decisions were made; but participants were not informed of the treatment offers until after planting decisions.

References

Anderson, Siwan. 2011. "Caste As an Impediment to Trade." *American Economic Journal: Applied Economics.* 3(1): 239-263.

Banerjee, Abhijit and Esther Duflo. 2000. "Reputation Effects and the Limits of Contracting: A Study of the Indian Software Industry." *Quarterly Journal of Economics.* 115(3): 989–1017.

Bardhan, Pranab K. 1983. "Labor-tying in a Poor Agrarian Economy: A Theoretical and Empirical Analysis." *Quarterly Journal of Economics,* 501–514.

Belloni, Alenxandre, Victor Chernozhukov, and Christian Hansen. 2012. "Inference on Treatment Effects after Selection amongst High-Dimensional Controls." *Review of Economic Studies*, 81(2): 608-650.

Belloni, Alenxandre, Victor Chernozhukov, and Christian Hansen. 2014. "High-Dimensional Methods and Inference on Structural and Treatment Effects." *Journal of Economic Perspectives*, 28(2): 29-50.

Collins et al. 2010. *Portfolios of the Poor.* Princeton University Press.

Fisman, Raymond and Inessa Love. 2003. "Trade Credit, Financial Intermediary Development and Industry Growth." *Journal of Finance*, 58(1): 353-374.

Gine, Xavier and Hanan Jacoby. 2015. "Markets, Contracts, and Uncertainty: A Structural Model of a Groundwater Economy." Working Paper.

Iyer, Raj and Antoinette Schoar. 2008. "The Importance of Holdup in Contract Negotiations: Evidence from an Audit Study." Working paper, MIT.

Jacoby, Hanan, Rinku Murgai and Saeed Rehman. 2004. "Monopoly Power and Distribution in Fragmented Markets: The Case of Groundwater." *Review of Economic Studies.* 71: 783-808.

Machiavello, Rocco and Ameet Morjaria. 2014. "Competition and Relational Contracts: Evidence from Rwanda's Coffee Mills." Working Paper.

Machiavello, Rocco and Ameet Morjaria. 2016. "The Value of Relationships: Evidence from a Supply Shock to Kenya Rose Exports." *American Economic Review*, forthcoming.

McMillan, John and Christopher Woodruff. 1999. "Interfirm Relationships and Informal Credit in Vietnam." *Quarterly Journal of Economics.* 114(4): 1285-1320.

Meyerson, Roger and Mark Satterthwaite. 1983. "Efficient Mechanisms for Bilateral Trading." Journal of Economic Theory. 29:265-281.

Townsend, Robert. 1994. "Risk and Insurance in Village India." Econometrica, 62(3):539-591.

FIGURES

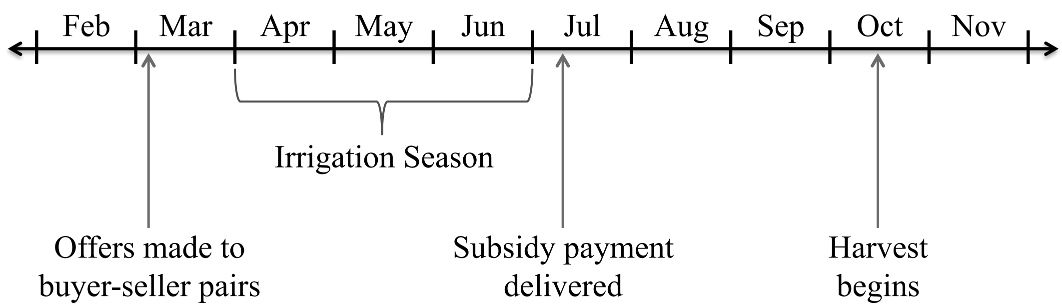

FIGURE 2. Experiment Timeline

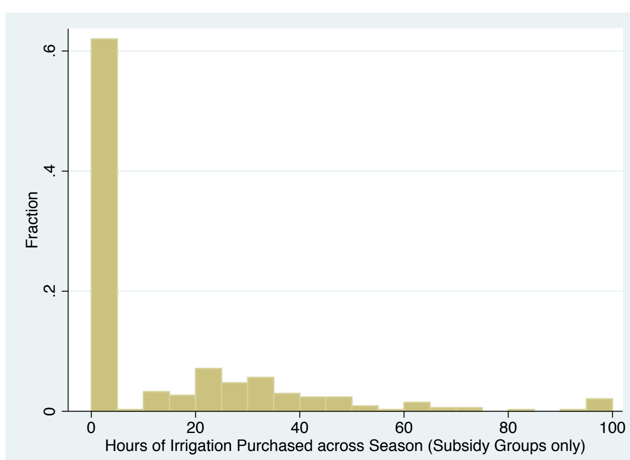

FIGURE 3. Distribution of Irrigation Hours

Notes: This figure shows the distribution of the total number of hours of irrigation purchased within buyer-seller pairs across the season in the full sample. The number of hours is topcoded at 100.

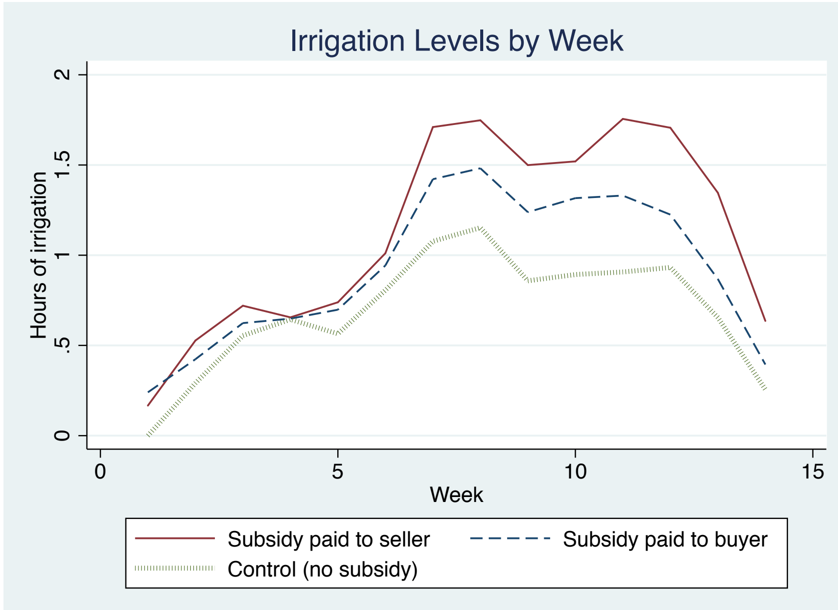

Figure 4. Average Irrigation Levels by Week

Notes: This figure shows the number of hours of irrigation within buyer-seller pairs. It plots the simple average number of hours purchased in each week of the experiment, separately for each treatment group. The plot lines are smoothed, using a lowess smoother of 0.35.

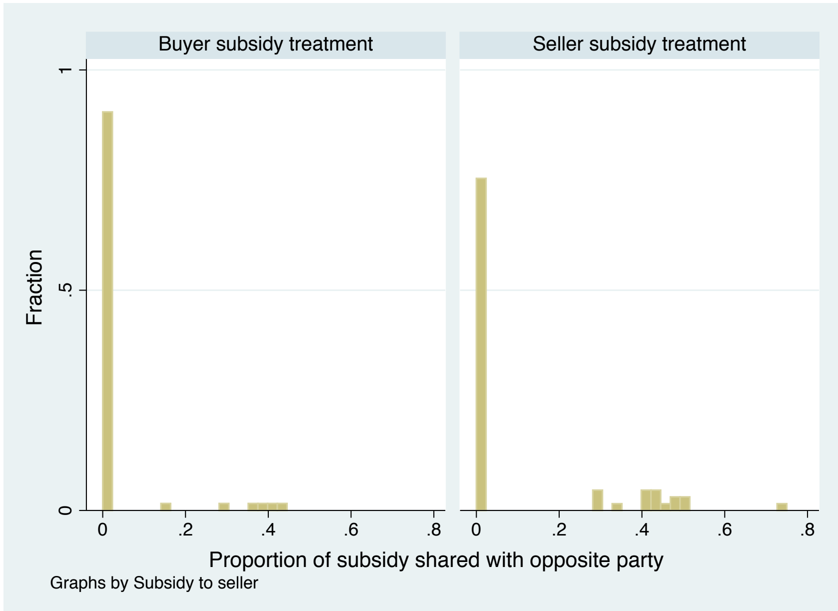

FIGURE 5. Ex-post Transfers: Sharing of the Subsidy

Notes: The sample is limited to pairs in the subsidy treatments who irrigated at least once and earned a subsidy. When the subsidy was delivered, each member of the pair was asked how the subsidy payment would be shared among them. The panels plot the distributions of proportions that would be shared with the seller in the Buyer subsidy case (left panel), and what proportion would be shared with the buyer in the Seller subsidy case (right panel).

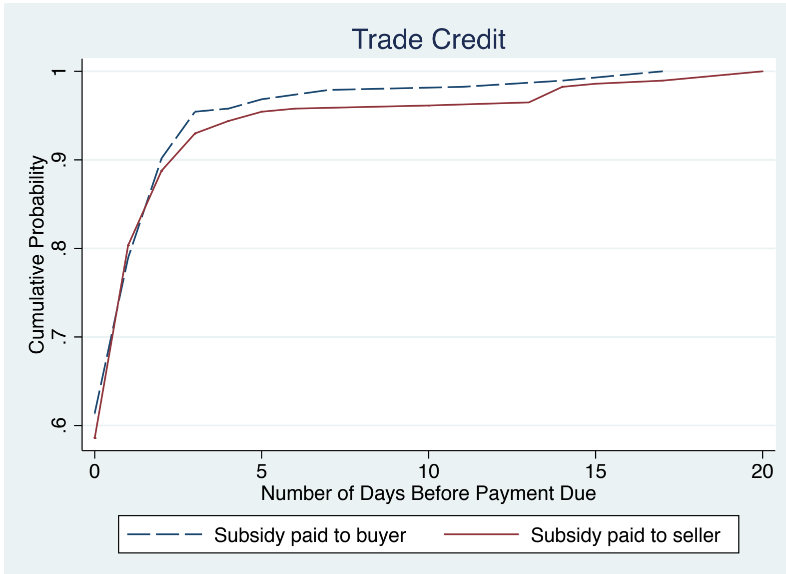

FIGURE 6. Trade Credit

Notes: Number of days before payment due = (Date when paymentwas made or expected) - (Date of irrigation), at time of weekly survey. Sample is restricted to pair-week observations where the pair irrigated. The figure plots the CDF of number of days of trade credit for each of the two subsidy groups.

TABLES

TABLE 1. Balance

	Full sample		Seller subsidy		Buyer subsidy		Diff.
	Mean	Std dev	Coeff.	p-val.	Coeff.	p-val.	p-val
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Seller characteristics							
Completed primary education	0.555	0.498	0.120	0.160	0.0640	0.451	0.407
Cash earnings over past year	36901	66996	5925.0	0.345	12151.7	0.260	0.529
Number of neighboring plots with borewell	2.205	1.364	-0.209	0.244	-0.360	0.045	0.288
No other neighboring plots have borewell	0.202	0.402	0.0493	0.323	0.0776	0.120	0.523
Number of neighbors growing sugarcane (potential buyers)	4.076	1.503	0.0971	0.663	0.00913	0.967	0.581
Borewell was operational at baseline	0.953	0.213	-0.0764	0.007	-0.0556	0.029	0.509
Depth of primary borewell (feet)	78.055	24.395	-5.728	0.094	-4.520	0.181	0.710
Well type: Dug well	0.618	0.487	-0.139	0.064	-0.0800	0.287	0.330
Well type: Brick well	0.150	0.357	-0.0706	0.288	-0.0679	0.307	0.952
Well type: Drilled well	0.213	0.410	0.0760	0.185	0.0236	0.664	0.286
Buyer characteristics							
Completed primary education	0.510	0.501	0.0546	0.490	0.0601	0.445	0.936
Cash earnings over past year	33199	19499	-3036.6	0.359	-1601.7	0.628	0.596
Number of neighboring plots with borewell	2.988	1.408	0.0233	0.896	0.0304	0.867	0.962
No other neighboring plot has well (except assigned seller)	0.142	0.349	-0.00757	0.866	0.0312	0.490	0.308
Irrigated in previous irrigation season	0.859	0.349	-0.0306	0.603	0.00189	0.974	0.504
Spent money on irrigation at any point in past year	0.937	0.243	0.119	0.020	0.0685	0.189	0.077
Social & market distance							
Buyer and seller have traded in past	0.628	0.484	0.0558	0.364	0.0284	0.640	0.589
Buyer and seller are same religion	0.895	0.307	-0.0132	0.751	-0.00573	0.888	0.820
Buyer and seller are same caste category	0.661	0.474	-0.0617	0.324	-0.0586	0.342	0.952
Buyer and seller are same subcaste	0.489	0.500	-0.0711	0.272	0.00337	0.958	0.166
Buyer's cultivated landholding higher than seller's	0.368	0.483	-0.131	0.163	-0.0109	0.907	0.104
Buyer's cash income higher than seller's last year	0.586	0.494	-0.104	0.225	-0.0172	0.834	0.218
Caste and religion							
Buyer is Hindu	0.852	0.355	0.0243	0.557	0.0429	0.291	0.556
Seller is Hindu	0.878	0.327	0.0115	0.735	0.0281	0.384	0.531
Seller is Scheduled caste/tribe	0.014	0.119	0.0207	0.079	0.0183	0.101	0.868
Buyer is Scheduled caste/tribe	0.036	0.186	0.00757	0.729	0.0201	0.381	0.552
Baseline survey selection							
Buyer was part of baseline survey sample	0.609	0.489	-0.00433	0.942	-0.0207	0.728	0.736
Seller was part of baseline survey sample	0.606	0.489	0.0603	0.304	0.0414	0.475	0.692

Notes: Cols (1)-(2) show sample mean and standard deviation for full sample (419 pairs). In Cols. (3)-(6), each row reports results from a separate regression of the covariate on dummies for Seller subsidy and Buyer subsidytreatments (Assignment to control is omitted category), and fixed effects for each village (strata). P-values for each coefficient are based on robust standard errors. Col (7) reports the p-value of an F-test for whether the Seller subsidy treatment coefficient equals the Buyer subsidy treatment coefficient in each of these regressions.

Table 2. Treatment Effects on Trade

Dependent variable	Hours of irrigation (OLS) (1)	Hours of irrigation (Tobit ME) (2)	Irrigated (dummy) (Logit ME) (3)	Ever traded (dummy) (Logit ME) (4)
Panel A: Irrigation Season				
Subsidy paid to water seller	0.832	1.478	0.1081	0.1852
	(0.281)***	(0.328)***	(0.0221)***	(0.0547)***
Subsidy paid to water buyer	0.332	0.872	0.0601	0.0881
	(0.251)	(0.313)***	(0.0214)***	(0.0551)*
P-value: Seller subsidy = Buyer subsidy	0.033	0.022	0.023	0.040
Observations	3,771 pair-weeks	3,771 pair-weeks	3,771 pair-weeks	419 pairs
Dependent var mean: Buyer subsidy group	1.177	1.177	0.151	0.354
Panel B: Full Subsidy Period				
Subsidy paid to water seller	0.669	1.236	0.0779	0.0716
	(0.234)***	(0.296)***	(0.0162)***	(0.0618)
Subsidy paid to water buyer	0.300	0.691	0.0426	-0.0152
	(0.206)*	(0.282)***	(0.0154)***	(0.0610)
P-value: Seller subsidy = Buyer subsidy	0.054	0.025	0.026	0.074
Observations	5,866 pair-weeks	5,866 pair-weeks	5,866 pair-weeks	419 pairs
Dependent var mean: Buyer subsidy group	0.943	0.943	0.116	0.360

Notes: Panel A is comprised of the irrigation season (May-June), and Panel B is comprised of all weeks when pairs were eligible to receive the subsidy (April-June). The omitted category in all regressions is Assignment to Control. The dependent variables are the number of hours of irrigation purchased by the buyer from his paired seller (OLS estimates in Col. 1, Tobit marginal effects in Col. 2), whether the buyer and seller traded that week (Col. 3, logit marginal effects), and whether the buyer and seller ever irrigated during the entire sample period (Col. 4, logit marginal effects). All regressions contain village fixed effects and baseline covariate controls. Standard errors are clustered by buyer-seller pair.

TABLE 3. Ex-Ante Transfers: Price Reductions

Dependent variable	Deviation from market price (OLS) (1)	Price discount (dummy) (OLS) (2)	Price discount (dummy) (Logit ME) (3)
Subsidy paid to water seller	-29.48	0.0818	0.1559
	(23.40)	(0.0587)	(0.0693)**
Subsidy paid to water buyer	-2.59	0.0023	0.0270
	(18.98)	(0.0563)	(0.0630)
P-value: Buyer subsidy = Seller subsidy	0.026	0.010	0.008
Observations (pair weeks)	642	642	404
Dependent var mean: Buyer subsidy group	4.17	0.082	0.125

Notes: Amount of price discount = (Market value - Amt charged). Market value = (modal price in village among control group)*hours of irrigation. Cols. (1)-(2) shows OLS estimates. Col. (3) reports estimated marginal effects from a logit regression. All regressions contain village fixed effects and the standard set of baseline covariate controls. The sample is restricted to pair-weeks where the pair irrigated. There were 7 villages where no discounts were ever offered; the regression in Col. (3) drops observations where the village fixed effects and controls perfectly predict no discount. Standard errors are clustered by pair.

TABLE 4. Trade Credit

Dependent variable	Any deferred payment (OLS) (1)	Any deferred payment (Logit ME) (2)	Number of days before payment due (OLS) (3)	Number of days before payment due (Tobit ME) (4)
Subsidy paid to water seller	0.0176	0.0293	0.0719	0.2690
	(0.0092)*	(0.0095)***	(0.0326)**	(0.0820)***
Subsidy paid to water buyer	0.0026	0.0044	0.0105	0.0655
	(0.0090)	(0.0084)	(0.0280)	(0.0332)**
P-value: Buyer subsidy = Seller subsidy	0.054	0.004	0.052	0.002
Observations (pair-weeks)	5,866	5,768	5,866	5,866
Dependent var mean: Buyer subsidy group	0.045	0.045	0.117	0.117

Notes: Number of days before payment due = (Date when paymentwas made or expected) - (Date of irrigation), at time of weekly survey. Cols. (1) and (3) shows OLS estimate; Cols. (2) and (4) report estimated marginal effects from logit and tobit regressions, respectively. All regressions contain village fixed effects and the standard set of baseline covariate controls. The regression in Col. (2) drops observations where the controls perfectly predict no trade credit. Standard errors are clustered by pair.

Table 5. Heterogeneity — Wealth

Dependent variable: Hours of irrigation

Interaction term	Seller has above median income (1)	Buyer has above median income (2)
1 Subsidy paid to water seller	1.380	1.354
	(0.461)***	(0.480)***
2 Subsidy paid to water seller x Wealth measure	1.660	0.554
	(1.014)*	(0.699)
3 Subsidy paid to water buyer	0.958	0.189
	(0.569)**	(0.489)
4 Subsidy paid to water buyer x Wealth measure	0.569	1.198
	(1.043)	(0.712)*
Hypothesis test p-values:		
Coefficient 1 = Coefficient 3	0.263	0.008
Coefficients 1+2 = Coefficients 3+4	0.004	0.236
Coefficient 2 = Coefficient 4	0.092	0.293
Observations (pair-weeks)	5,866	5,866
Dependent var mean: Buyer subsidy	0.943	0.943

Notes: Omitted treatment category is Assignment to Control. Regressions show estimated marginal effects from tobit regressions. Wealth measure computed using self-reported baseline income; median threshold in both columns is based on median income of water buyers. Regressions include village fixed effects and the standard set of baseline covariate controls. Standard errors are clustered by pair.

TABLE 6. Heterogeneity — Potential Correlates of Informal Enforcement Mechanisms

Dependent variable: Hours of Irrigation

	Market interactions				*Social distance*			
Interaction term	Buyer and seller have traded in past (1)	Buyer has bought irrig from a neighbor (2)	Seller has sold irrig to a neighbor (3)	Buyer has no other nearby sellers (4)	Buyer and seller have visited other's home (5)	Buyer and seller are same religion (6)	Buyer and seller are same caste (7)	Buyer and seller are same subcaste (8)
Panel A: Effects on ability to take advantage of subsidy								
1 Assigned to Subsidy group	-0.260	0.310	0.538	1.073	0.119	-0.669	1.170	1.560
	(0.500)	(0.567)	(0.386)	(0.334)***	(0.420)	(0.985)	(0.681)*	(0.594)***
2 Assigned to Subsidy group x Interaction term	1.891	1.136	1.182	-0.033	1.315	1.907	-0.137	-0.781
	(0.656)***	(0.676)*	(0.594)**	(0.832)	(0.631)**	(1.048)*	(0.754)	(0.676)
p-value: Coefficients 1+2 = 0	0.000	0.000	0.000	0.175	0.001	0.000	0.002	0.027
Panel B: Differential effects by subsidy treatment								
1 Subsidy paid to water seller	-0.144	0.510	0.810	1.400	0.543	-0.617	1.410	1.726
	(0.548)	(0.607)	(0.417)*	(0.359)***	(0.457)	(1.203)	(0.710)**	(0.612)***
2 Subsidy paid to water seller x Interaction term	2.096	1.245	1.240	-0.559	1.087	2.164	-0.069	-0.550
	(0.707)***	(0.716)*	(0.648)*	(0.934)	(0.661)*	(1.266)*	(0.797)	(0.708)
3 Subsidy paid to water buyer	-0.334	0.219	0.292	0.767	-0.277	-0.691	0.963	1.416
	(0.562)	(0.602)	(0.427)	(0.353)**	(0.484)	(1.009)	(0.701)	(0.621)**
4 Subsidy paid to water buyer x Interaction term	1.671	0.928	1.168	0.317	1.523	1.683	-0.184	-0.930
	(0.707)**	(0.726)	(0.633)*	(0.871)	(0.691)**	(1.077)	(0.782)	(0.709)
Hypothesis test p-values:								
Coefficient 1 = Coefficient 3	0.699	0.482	0.130	0.015	0.052	0.942	0.277	0.378
Coefficients 1+2 = Coefficients 3+4	0.021	0.041	0.074	0.709	0.167	0.025	0.058	0.026
Coefficient 2 = Coefficient 4	0.446	0.538	0.881	0.218	0.387	0.650	0.822	0.413
Coefficients 1+2 = 0	0.000	0.000	0.000	0.334	0.001	0.000	0.000	0.003
Coefficients 3+4=0	0.001	0.003	0.003	0.173	0.008	0.003	0.030	0.188
Interaction term sample mean	0.628	0.616	0.406	0.136	0.594	0.895	0.613	0.489

Notes: Omitted treatment category is Assignment to Control. All regressions show estimated marginal effects from tobit regressions. Buyer has no other potential sellers equals 1 if no other plots of land around the buyer's plot have a boring on them. All regressions include village fixed effects and the standard set of baseline covariate controls. Standard errors are clustered by pair. N=5,866 pair

TABLE 7. Treatment Effects on Yields

Dependent variable	*Total irrigation*	*Yields index (standard deviations)*	
	(1)	(2)	(3)
Subsidy paid to water seller	6.14	0.392	0.477
	(2.13)***	(0.162)**	(0.185)***
Subsidy paid to water buyer	3.32	0.118	0.161
	(1.93)*	(0.141)	(0.162)
P-value: Buyer subsidy = Seller subsidy	0.117	0.0518	0.0561
Interactions of treatment with fallow plot indicator?	No	No	Yes
Observations	419	419	419
Dependent var mean: Buyer subsidy	14.61	0.126	0.003

Notes: The dependent variable in Col. (1) is the total hours of irrigation purchased by the buyer-- from his paired seller or any other seller. Col (1) shows Tobit marginal effects. Dependent variable in Cols (2)-(3) is a composite index of crop yields, measured as standard deviations of crop yields for each crop in the Control group. Col. (3) includes an interaction of each treatment indicator with a dummy for whether the plot was fallow before the start of the experiment. (For administrative reasons, plots were chosen by us in the previous year before planting decisions were made; but participants were not informed of the treatment offers until after planting decisions). All regressions include village fixed effects and the standard baseline covariate controls. Robust standard errors reported.

Appendix A. Supplemental Figures and Tables

Table A1. Robustness - Specification Checks

Dependent variable: Hours of Irrigation within Pair

	(1)	(2)	(3)	(4)	(5)
Subsidy paid to water seller	0.641	0.725	0.832	1.203	0.843
	(0.310)**	(0.306)**	(0.281)***	(0.404)***	(0.270)***
Subsidy paid to water buyer	0.321	0.261	0.332	0.389	0.373
	(0.275)	(0.280)	(0.251)	(0.317)	(0.240)
P-value: Buyer subsidy = Seller subsidy	0.194	0.0509	0.0329	0.0231	0.0427
Controls collected for all households?	No	Yes	Yes	Yes	No
Controls from full baseline survey?	No	No	Yes	Yes	No
Identification using only HHs with full baseline?	No	No	No	Yes	No
Only post-LASSO controls?	No	No	No	No	Yes
Observations (pair weeks)	3771	3771	3771	3771	3771

Notes: OLS regressions. Sample is all pair-weeks during irrigation season. All regressions have fixed effects for each village (strata). Col. (2) adds the covariate controls from the balance table that were collected for all households in the sample. Col. (3) further adds the controls from the full baseline survey (conducted for 61% of households); for households that did not receive the full baseline, these additional covariates are coded as zero and the regression contains dummies for whether buyer and seller, respectively, were in the full baseline sample. Col. (3) therefore includes all covariates in the balance table, and corresponds to the main specification in the paper. Col. (4) further adds interactions of each treatment indicator with a dummy for whether either the buyer or seller wasn't in the baseline survey sample; the displayed coefficients therefore reflect treatment effects only for households where baseline surveys were conducted. Col. (5) only includes controls selected using the post-double selection method, using LASSO to select those controls from the balance table with the most predictive power for outcomes. Standard errors clustered by pair. Note that for each treatment, all specifications give coefficients that are within 95% confidence intervals of each other.

TABLE A2. Crowd Out: Trade with Others

	(1)	(2)	(3)
Panel A -- Crowd Out for Seller: Transactions with Other Buyers			
	Number of buyers	Number of buyers	Total payments received
Subsidy paid to water seller	-0.1128	-0.1091	-50.4
	(0.0724)*	(0.0444)**	(13.6)***
Subsidy paid to water buyer	-0.0205	-0.0196	33.9
	(0.0825)	(0.0065)***	(33.3)
P-value: Buyer subsidy = Seller subsidy	0.076	0.011	0.004
Estimator	OLS	Tobit	Tobit
Observations (pairs)	419	419	419
Dependent var mean: Control group	0.232	0.232	213
Panel B -- Crowd Out for Buyer: Transactions with Other Sellers			
	Number of sellers	Number of sellers	Total payments made
Subsidy paid to water seller	-0.0096	-0.0149	0.7316
	(0.0780)	(0.0064)	(5.47)
Subsidy paid to water buyer	0.0392	0.0293	19.7726
	(0.0781)	(0.0251)	(16.39)
P-value: Buyer subsidy = Seller subsidy	0.428	0.000	0.004
Estimator	OLS	Tobit	Tobit
Observations (pairs)	419	419	419
Dependent var mean: Control group	0.317	0.317	49

Notes: The table reports effect on the extent of trade with other parties (other than the paired buyer or seller). The omitted category in all regressions is Assignment to Control. Col. 1 reports OLS estimates; the remaining columns report Tobit marginal effects. All regressions contain village fixed effects and the standard set of baseline covariate controls. Robust standard errors.